Built on wild fancies which thy name surround?
Or doth the story of thy classic ground
With the stern facts of Nature's face agree?
What if no tongue may tell!—thy halo fair
Still lingers round the isles which slumber there."

("Lyra Devoniensis," p. 135).

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Indian Rainfall

As Dr. Hunter has been good enough to mention my name in his letter to NATURE (vol. xvii. p. 59) in connection with a comparison of the rainfall of Northern India and the sun-spot period, I may, I trust, be allowed to express my opinion regarding the validity of some of the conclusions he draws therefrom. In the first place I would remark that Dr. Hunter's idea of the *winter* rainfall of Northern India being due to the *immediate rebound of the summer monsoon* from the Himalayan barrier is at variance with facts in the meteorology of the country. The rebound ought to take place directly the monsoon vapour-current impinges upon the Himalaya, *i.e.*, in the *summer*. In fact, it is by a succession of oblique rebounds from this impassable barrier that the monsoon is gradually reflected towards the N.W.P. and the Punjab.

The *winter* rains, on the contrary, have nothing to do with the monsoon, being, as is well known, due to a branch of the anti-trade, which, descending in the Punjab, is deflected by the Himalayas towards Behar and Bengal, occasionally reaching Calcutta, lat. 22° 35′ N.

Now between the rains of the summer monsoon and those of the anti-trade (or anti-monsoon as it is occasionally called), there is a well-marked interval of bright, clear, settled weather for two or three months throughout Northern India. After this interval the skies again become clouded, and about Christmas, or shortly after, the gentle but soaking rain of the cold weather sets in, and is repeated at intervals up to the end of March. It is evident, therefore, that the two currents, monsoon, and anti-trade, are totally unconnected with each other; and hence arises the desirability, especially in a question like the present, in which its secular variations are being discussed, of completely separating the rain of the former from that of the latter vapour current. I cannot but think that it is his omission to allow for these vapour currents that has led Dr. Hunter to offer such an erroneous explanation of the results obtained. According to him, copious precipitation should take place in the interval (October to December) between the two seasonal falls, during which clear weather is invariably present. It may be added that the period (December to April) which I took to comprise the winter fall, commenced after this interval.

The real explanation of the direct variation of the summer, and the inverse variation of the winter rainfall of North India, with the sun-spot period, is the hypothesis which first led to its verification coincidently and independently, by Mr. Hill and myself.

To enter upon a complete exposition of this hypothesis would occupy too much of your valuable space, but as it has been found to explain most of the anomalies which have hitherto proved such powerful obstacles (especially in extra-tropical regions) to the universal extension of the theory of sun-spot influence (I use the term advisedly) on the different elements of terrestrial meteorology, I will here briefly indicate its general outlines for the benefit of other workers in the same field of investigation.

The hypothesis, to start with, assumes *the solar radiation to vary inversely with the sun-spot frequency*.

It then takes account of the probable effects of such a variation upon the vapour-bearing currents throughout the globe with respect to velocity, direction, season, and latitude. According as trade, anti-trade, monsoon, or anti-monsoon, prevail (1) at different places at the same season, (2) at the same place at different seasons, so will specifically distinct effects arise both from the amount of vapour brought and its conditions of precipitation, to determine which, not only the general conditions introduced by latitude and season, but the local and peculiar meteorological functions of the region must be carefully studied.

Now as the principal effect of a secular change in solar radiated heat must be to cause a similar direct secular change in the normal convection currents of the atmosphere, we may expect the tropical trade-wind and monsoon regions to furnish us with some evidence, whether direct or indirect, in favour of the above hypothesis.

Little direct evidence has at present been adduced besides that given by Mr. Hill from a comparison of wind velocity in the N.W.P. (NATURE, vol. xvii. p. 505). A good deal of indirect evidence, however, is furnished in the monsoon regions by the occurrence of abnormal droughts and floods in contiguous districts (the drought in the N.W.P. and floods in Assam and Burmah last year were good examples of this kind) at the time of minimum sun-spot, when the velocity of the current being increased it travels in a more contracted channel, and, by a more equable distribution of rain at the time of maximum sun-spot, when the velocity of the current being decreased, it is more liable to extend laterally. In the trade-wind regions similar evidence is furnished by the fact of a deficiency of rain and cyclones at the time of minimum sun-spot, with a corresponding excess of both at the time of maximum sun-spot. The augmented velocity of the wind currents at the former epoch, preventing the formation of local areas of condensation and precipitation, and therefore (according to Messrs. Blanford and Eliot's theory of cyclone generation) of cyclones and their accompanying downpours; while the diminished velocity at the latter epoch favours the same.

Finally, the anti-trade which in its seasonal shifts north and south traverses the entire temperate zone, in the winter bringing rain to North India, Palestine, Madeira, California, &c., and in the summer to Northern Europe and Siberia, should give signs of a secular change in intensity and humidity, corresponding according to the hypothesis inversely with the sun-spots. In the summer, when large continental areas like Europe are more immediately under the direct influence of solar heat, local convection currents being set up will tend to disturb and complicate the effect of any general change in the strength of the anti-trade. In the winter, on the other hand, the obliquity of the incidental solar rays leaves the anti-trade in undisputed possession of the field. At this season, therefore, there should be a marked variation in the rainfall of the temperate zone, more particularly in those regions between 25° and 40° N. and S. lat., where the rainfall of *this season* is the chief rainfall of the year, corresponding *inversely* with the sun-spots. Even in those regions where the rain falls at all seasons, if we pick out the winter from the total annual falls, as was done by Mr. Draper, for New York (NATURE, vol. xvii. p. 15) in accordance with Mr. Hill's admirable suggestion (vol. xvi. p. 505), the results favour the hypothesis. But they do this in a far more marked manner where the rain of the entire year falls during the winter months, as in the Mediterranean and at Jerusalem, which have consequently hitherto been considered by Dr. Jelinek and Mr. Meldrum to afford strong evidence against the theory of a direct connection between rainfall and sun-spots. The inverse variation of the winter rainfall of Northern India is only another example of the same law, and shows how extremely important it is to analyse the seasonal variations separately before deciding the question by a mere cursory glance at the *total annual* falls. The apparent anomalies which Dr. Hunter finds presented in the North American rainfalls are, I think, due to his having compared the total annual falls. If he, and other investigators will only take the hint dropped by Mr. Hill, and which I cordially endorse, of comparing the seasonal falls separately, they will find, I think, that while the summer rainfalls of the temperate zone show either a non-periodic variation, or symptoms of one coinciding directly with the sun-spots, the *winter* falls will in general show unmistakable signs of a variation coinciding *inversely* with that of sun-spot frequency and area.

E. D. ARCHIBALD

Sun-spots and Rainfall

I HAVE read with much interest Dr. Meldrum's paper on Sun-spots and Rainfall in NATURE (vol. xvii. p. 448), particularly that part of it in which Dr. Hunter's method of discussing the rainfall of Madras is criticised, and a method of inquiry in sun-spot researches is proposed. This method is, so far as I am aware, a new one, and as such, is deserving of careful examina-

tion as to how far it is applicable to the data submitted for discussion.

Dr. Hunter published the data for discussing the rainfall at Madras during the six sun-spot cycles, ending 1876, these being all the available data for Madras. As regards the sun-spots, we certainly have no *positive* data earlier, at least, than these cycles, whatever value may be attached to the approximate earlier figures supplied by Dr. Wolf. As regards, therefore, both the elements under discussion, viz., the sun-spots and the rainfall, the period discussed by Dr. Hunter represents the whole of the cycles for which material is available.

In dealing with this period, Dr. Hunter divides it into six equal cycles of eleven years each, this being substantially the average duration of the sun-spot cycles. I have arranged the relative numbers published in Wolf's last list (*Wolf. Astronomische Mittheilungen*, pp. 35-37), according to the cycles adopted by Dr. Hunter, with the result that all the six minimum years of sun-spots occurred either in the first year of the cycle, or in one of the immediately adjoining ones on either side of it, viz., in the second or in the eleventh years. As regards the years of maximum sun-spot, five out of the six occurred in the fifth or sixth years of the cycle, and the remaining year of maximum sun-spots occurred in the eighth year.

In his paper Dr. Meldrum states that as the sun-spot cycles are not all of the same length, it is evident that, by starting from any one year and going backwards over a long period, always using the same fixed number, a maximum and a minimum year might fall into the same group, and it was to obviate the occurrence of this contingency which the above analysis of Dr. Hunter's method shows did not occur during the period discussed by him, that Dr. Meldrum has proposed his new method as a more accurate mode of discussing the data.

To test the value of this new method of inquiry, I have arranged Wolf's relative numbers of sun-spots in accordance therewith, the maximum year of sun-spots of each cycle being placed in the sixth year, the minimum years being marked with an asterisk, and the "mean cycle" of eleven years being calculated from the thirteen years in the manner described by Dr. Meldrum :—

Year.	1811-23	1824-36	1832-44	1843-55	1855-67	1865-77	Means	Mean cycle.	Year of cycle.
1	1·6	8·1	26·3	*13·1	7·7	31·4	14·7		
2	4·9	16·2	*9·4	19·3	*5·1	14·7	11·6	14·9	1
3	12·6	35·0	13·3	38·3	22·9	*8·8	21·8	25·4	2
4	16·2	51·2	59·0	59·6	56·2	36·8	46·5	48·8	3
5	35·2	62·1	119·3	97·4	90·3	78·6	80·5	77·0	4
6	46·9	67·2	136·9	124·9	94·8	131·8	100·4	91·1	5
7	39·9	67·0	104·1	95·4	77·7	113·8	83·0	83·0	6
8	29·7	50·4	83·4	69·8	61·0	99·7	65·7	65·6	7
9	23·5	26·3	61·8	63·2	45·4	67·7	48·0	49·0	8
10	16·2	*9·4	38·5	52·7	45·2	43·1	34·2	34·6	9
11	6·1	13·3	23·0	38·5	31·4	18·9	21·9	24·6	10
12	3·9	59·0	*13·1	21·0	14·7	11·3	20·5	22·5	11
13	*2·6	119·3	19·3	7·7	*8·8	*7·0	27·5		

It will be seen from this table that with this arrangement the year of minimum sun-spots has occurred on the tenth, twelfth, thirteenth, first, second, and third years. By Dr. Hunter's arrangement the minimum years fell within a compact group of three consecutive years out of a cycle of eleven, whereas by Dr. Meldrum's arrangement they are scattered over seven years out of a cycle of thirteen. Further, I find that in the second cycle what is virtually a maximum year (viz., 1836 with 119·3 of sun-spots) fell within his minimum group, or in the thirteenth year. This is precisely the result which the method was designed to avoid, but as to the occurrence of which there was not an approach under Dr. Hunter's arrangement.

Again, if the same relative numbers of Wolf be arranged as Dr. Meldrum proposes, so that the year of minimum sun-spots of each cycle be placed in the ninth year of the thirteen years, it will be found that the maximum years are scattered over the twelfth, thirteenth, first, second, third, and fifth years of the series. By Dr. Hunter's method of arrangement five out of the six maximum years fell in the fifth and sixth years of the series, while the remaining one fell in the eighth year, thus again presenting a compact group, whereas Dr. Meldrum's method scatters them over more than half of his series of thirteen years.

An objectionable feature of this new method is the necessary repetition of figures which it involves. Thus, in the table given above, embracing six cycles, nine minimum years occur; and in the table in which all the minimum years are so arranged as to stand in the ninth year of the cycle, nine maximum years also occur, so that if the Madras rainfall were discussed by this method, the averages would be computed from tables in which the maximum and minimum years occur eighteen instead of twelve times.

Mr. Meldrum's method might be improved if he entirely struck out the first and thirteenth years of the thirteen years series, and simply "bloxamed" the remaining eleven years for the years of his "Mean Cycle;" that is, made the first of these years the mean of the eleventh, first and second; the second year the mean of the first, second and third. Even, however, with this change the method is inferior to that employed by Dr. Hunter, and the force of this statement will be the more readily recognised if it be kept in mind that we have no *positive* data from which the relative numbers of the sun-spots can be calculated prior to the time when Schwabe began his great work of sun-spot observation.

Edinburgh, April 22 ALEXANDER BUCHAN

Trajectories of Shot

MR. NIVEN was perfectly welcome to make use of my experiments and tables, as he has done, in trying to devise new methods of calculating trajectories of shot. And when he had satisfied himself that his methods possessed some advantages over others, he required no excuse whatever for their publication. But I altogether object to Mr. Niven's rule for finding v_β being connected in any way with the mode of calculation adopted by me. I beg, therefore, to place side by side Mr. Niven's rule, to which I object, and my rule, which I make use of, and so leave the matter. Mr. Niven says respecting v_β :—

"The first steps in our work must be to guess at it. The *practised calculator* can, from his experience, make a very good estimate. Having made his estimate he determines k. He uses the value of k in equation (a), and if he gets the velocity he guessed at, he concludes that he guessed rightly, and that he has got the velocity at the end of the arc. If equation (a) does not agree with him he makes another guess, and so on till he comes right."

The following is the course I pursue to find v_β. Refer to the table of coefficients and take out the value of k_a corresponding to the initial velocity v_a. Substitute in equation (a) and find a *first* approximate value of v_β. Now determine the mean value of k between v_α and v_β just found, substitute in equation (a), and thus find a *second* approximate value of v_β, which will generally be found sufficient. Otherwise adjust by proportional parts.

In this way the value of v_β is found accurately on the supposition that k has remained constantly at its mean value between v_a and v_β. Here the operations are of the simplest kind, and no guessing or practised calculator is required. And with a view to diminish the tedium of making these calculations, tables of $\Sigma\,(k)$, $\Sigma\,(k \div g)$, $(1000 \div v)^3{}_a$, &c., have been calculated and printed, but their publication has been delayed on account of the experiments proposed to be made with low velocities.

Since Mr. Niven described the process of guessing as "*extremely* dangerous," there can be no doubt that the epithet was "extreme." As I supposed, he is not prepared to supply me with a single practical case where his condition of danger is satisfied. And if a case cannot be found then the objection falls to the ground. Whether we consider the range of values of k for spherical or ogival-headed shot, for velocities above 1,200 f.s., we shall find that $\frac{dk}{dv}$ lies between the limits o and $-$ 0·09, or, where k is a mean over an arc, between o and $-$ 0·05 about. And it is the smallness of this tabular value which renders it difficult, if not impossible, to satisfy Mr. Niven's condition of danger. But if this quantity had not been small, then the cubic law could not have been used even approximately. Mr. Niven is at liberty to take shot of any size used in practice, moving at any attainable velocity beyond 1,200 f.s., and the coefficients of resistance for either spherical or ogival-headed projectiles. The objection is Mr. Niven's, and he must take the onus of supporting it if he still thinks it of value.

I regret to have to write anything in opposition to Mr. Niven's paper, because in all other respects it appears to me a valuable contribution to the science of ballistics. F. BASHFORTH

Minting Vicarage, April 17